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A Review Essay

Politicians Make Bad Scientists

William Havender

HOW GOOD A SCIENTIST is government? The question goes to the very heart of the public's readiness to accept government regulation. For this acceptance rests on the premise of agency impartiality and expertise—the automatic assumption that agency officials, lacking any direct financial interest and having access to the best professional advice, will arrive at objective decisions serving the public interest, based on a cool evaluation of the most reliable information available.

But that there may be legitimate grounds for questioning this assumption is shown in an article written by Sir Richard Doll and Richard Peto, two of the world's foremost authorities in cancer epidemiology, and carried in the June issue of the *Journal of the National Cancer Institute* ("The Causes of Cancer: Quantitative Estimates of Avoidable Risks of Cancer in the United States Today"). The article is an excruciatingly thorough and sober evaluation of all the evidence relating to the proportion of cancers now occurring in the United States that may be attributed to external causes—smoking, diet, occupation, pollution, and the like. This massive article (some one hundred pages long) is a landmark in the field of cancer epidemiology, and will remain so for the next decade, until new data are gathered that confirm or refute its cautious conclusions.

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In the course of this review, the authors direct their attention to two recent U.S. government reports dealing with the origins of cancer in the United States. One is a report—sometimes referred to as the "Califano," or "OSHA," report—which focused on the fraction of cancers that could be caused by on-the-job chemical exposures. The other is an interagency report to President Carter by the Toxic Substances Strategy Committee ("Toxic Chemicals and Public Protection," 1980), which claimed that the overall incidence of cancer in the United States was rising steadily, even *after* correction for the effects of smoking.

The Califano report, which then-Secretary of Health, Education, and Welfare Joseph Califano publicized in a speech to an AFL-CIO conference in September 1978, alleged that as much as 38 percent of *total* cancers in the United States might be occupationally related. This claim was stunning, because all previous estimates had placed the proportion in the range of 1 to 5 percent. That is, the new estimate was some *ten times higher* than the prevailing consensus.

The aberrancy of the claim would ordinarily have inspired careful examination by other scientists of its underlying methodology. But the agencies concerned with regulating exposure to carcinogens did not wait upon the final judgment of the unhurried, somewhat stately process of peer review. After Secretary Califano publicly embraced the report's conclusions, it took on a life of its own and has been rattling around government agencies ever since, subtly influencing policy initiatives and the public debate. In particular, it was uncritically accepted as the basis for a portion of the 1980 report of the Toxic Substances Strategy Committee (TSSC), prepared under the auspices of the Council on Environmental Quality.

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Peer review of the Califano report was slowed by two unusual factors. One was that the report had no identified authors, and hence

no particular person(s) had the responsibility for defending it. Instead, it listed ten researchers at the National Cancer Institute, the National Institute of Environmental Health Sciences, and the National Institute for Occupational Safety and Health as "contributors" (at least seven of whom have since disavowed its sensational claims). The second factor was that the report was never submitted for publication in any scientific journal (and hence avoided the critical scrutiny of editors and reviewers), but was merely filed at the Occupational Safety and Health Administration (OSHA) in a post-hearing record on September 15, 1978.

In their *JNCI* article, Doll and Peto found this report to be mortally flawed, and the central defect, far from being esoteric and understandable only by those with advanced degrees in statistics, is simple and blatant. The defect, which Doll and Peto say "might fairly be described as a confidence trick," is this: cancer risks from exposure to particular chemicals were estimated from epidemiology studies on small groups of workers that had had *extremely high* exposures lasting over periods of *many years,* and these risk estimates were then applied to the *total* number of workers having *any* exposure *whatever,* no matter how little the dose or how short the duration. Say Doll and Peto: "This disregard of both dose and duration of exposure is indefensible and produces risk estimates which are more than ten times too large. . . ." They go on to stress that the "error is quite as gross as to suppose that a non-smoker who works in a factory where other people smoke cigarettes will have a 20 percent risk of lung cancer because he is passively exposed to some cigarette smoke and 20 percent of heavy cigarette smokers get lung cancer." Moreover, the report was probably written "for political rather than scientific purposes. . . ." And, even though its conclusions are widely cited, "the crucial parts of the arguments for these conclusions have, perhaps advisedly, never been published in a scientific journal. . . . Unless they are, with proper attribution of responsible authorship, we would suggest that [it] should not be regarded as a serious contribution to scientific thought. . . ."

The other government study that Doll and Peto discuss, the 1980 TSSC report, found that "even after adjustments for age, . . . recent figures show that both incidence (new cases) and mortality (deaths) rates are increasing" by some 1 to 2 percent a year, and further that "when the effects of cigarette smoking are corrected for, the recent trends in incidence show an increase." The discussion of occupational cancers in this report was based largely on the conclusions of the Califano report, which as we have seen were gravely fallacious. But Doll and Peto discuss two *additional* defects in the methods used. One involves biases in inferring overall trends by comparing the cancer incidence rates found in the third National Cancer Survey of 1969–71 and in the Surveillance, Epidemiology, and End Result Program of 1973–77. This matter is so complex (the interested reader should see Appendix C of their paper) that I will only note here Doll and Peto's conclusion: that comparing these two studies with the object of inferring time trends in the occurrence of cancer is "the one comparison of incidence rates that seems completely unreliable" and that "[q]uite fantastic and irregular variations in incidence are suggested by such comparisons."

The second new flaw in the TSSC report is easier to grasp. It concerns the report's claim that lung cancer incidence rates are rapidly increasing even *after* allowance is made for smoking. This conclusion was the result of overlooking the *lagged* nature of cancer induction from environmental agents—the fact that decades may elapse between the commencement of exposure to a carcinogen and the manifestation of cancer symptoms. This phenomenon has been particularly thoroughly documented in the case of cigarette smoking, where lung cancer rates in the U.S. and English populations did not start their rapid increase until twenty or thirty years *after* cigarette smoking became popular.

As well established as this basic fact is, the Toxic Substances Strategy Committee somehow managed to ignore it, and based its analysis, in the words of Doll and Peto, "on the absurd assumption that if American smoking habits . . . had been constant from the 1960s to the 1970s, there would have been no large trends in lung cancer over this period!" Driving their point home, the authors note: "This extraordinary failure to expect any large increases due to the 'successive generation effect' is a simple scientific error to be corrected,

rather than a new scientific hypothesis to be considered, because massive 'successive generation effects' have been seen or are being seen in every country that has adopted cigarette smoking on a large scale before the middle of this century." Doll and Peto's own conclusion is that the overall trends in age-adjusted lung cancer rates (which, to be sure, have been on the rise, especially among women) can be plausibly explained by cigarette smoking, and that these trends do not, by themselves, supply evidence strongly implicating any other major contributor to the incidence of lung cancer.

In these two instances, then, governmentally generated reports making aberrant and, indeed, quite fantastic claims about the causation of cancer—a matter having enormous regulatory impact since these claims directly affect the scope and urgency of actions by OSHA, the Environmental Protection Agency, and other agencies—got palmed off as possessing the authority and majesty of science. And this took place even though these reports had escaped the process that is the essence of the scientific endeavor and the source of its prestige, namely, being submitted for, and surviving, critical evaluation by the scientific community. That evaluation, now rendered at last, is withering and scornful, and it eviscerates the chief conclusions of these reports.

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It is hard to say just how many flawed government initiatives were set in motion by these two reports. But it is perhaps less important that the reports were the cause of distortion than that they were the result of it. For they call into question not merely the government's expertise, but its impartiality as well. It will not take many more such examples to shred completely the public's faith, already tattered by the Love Canal, nitrite, and saccharin imbroglios, in the uses that Washington bureaucrats make of science. ■